the Commission as tend to justify the position taken up by the Society, leaving the advocates of vivisection, as it tells us in a prefatory note, to give publicity to such parts of the evidence as favour their views. We have selected the witnesses quoted by this Society as giving evidence upon the extension and abuses of vivisection, in order that we might not run the risk of being misled by partial statements and of under-estimating the extent to which the practice prevails in this country.

The first of these, Dr. Acland, observes that the number of persons in this and other countries who are becoming biologists without being medical men is very much increasing; but beyond this statement, in which he appears to have in view professional physiologists rather than occasional experimenters, there is nothing in his evidence to lead to the belief that vivisection is practised to any extent by the latter class. Mr. G. H. Lewes tells us that so far from ignorant people exercising their fancy in cutting up live animals, even medical students are extremely reluctant to perform experiments at all on account of the trouble involved in doing so. Sir William Ferguson states that the impression on his mind is that experiments are done very frequently in a most reckless manner; but when we look for the grounds of this belief we cannot find anything except the accounts given by students of experiments they had seen during lectures. We should have thought a man of Sir William's experience would not have trusted to such hearsay evidence without farther investigation, knowing, as he must do, how students delight to exaggerate and to tell frightful stories of the dissecting-room for the pleasure of seeing their mother's or sister's eyes grow wide with horror at the tale. The evidence of other witnesses shows that such exaggeration must have been practised here, and that no such experiments as Sir William describes have been performed in any medical school in this country. But this witness is of opinion that it is only in laboratories and schools that vivisection is carried on, as in this country surgeons do not employ it for the purpose of acquiring dexterity, and he thinks there is not much amateur physiology. Such evidence from an active opponent of vivisection goes far to show that the number of occasional experimenters cannot be great, and that the practice of vivisection is almost entirely confined to the fifteen or twenty persons alluded to by the Commissioners. Small as this number is, we would have considered it right to legislate if anything like wanton cruelty had been shown to be practised by them; but the Secretary of the Royal Society for the Prevention of Cruelty to Animals admits that he does not know a single case of the sort, and that in general English physiologists have used anæsthetics where they think they can do so with safety to the experiment. Such being the case, it seems to us that the objections raised to legislative interference by several witnesses carry great weight. Those made by Mr. John Simon are especially worthy of consideration, not only on account of his well-known ability and clear-sightedness, but because his official position has given him a better opportunity of becoming acquainted with the working of laws and of forming a correct judgment regarding the probable operation of any proposed bill than other witnesses who are constantly engaged either in the laboratory or with the cares of practice. The opinion he expresses that incompetent experimenters, careless of the sufferings they inflict, do not exist as an appreciable class in this country, is borne out by the evidence we have already referred to, and it does seem hard that physiologists of high reputation and unblemished character should be treated as a dangerous class, and should be licensed and regulated "like publicans and prostitutes under the licensing system;" Mr. Simon considers that it would afford facilities for the persecution of physiologists, and would enable those who are so inclined to hold them up individually to popular odium. That the inclination is not wanting is shown by the advertisements of the Society for the Abolition of Vivisection constantly appearing in the daily papers. In these an attempt is made to destroy whatever medical practice the authors of the "Handbook for the Physiological Laboratory" may have, and by thus reducing their means of livelihood to starve them as far as possible. This is done by representing them as so hardened by their pursuits and so callous to suffering as to be unfit for attendance at a sick-bed; although we learn from the evidence of Prof. Rolleston and Mr. Simon that two of them at least are exceedingly kindhearted men, and the other two have devoted themselves to researches having an unusually direct bearing on the prevention of disease or the alleviation of suffering.

Much and careful consideration is therefore wanted lest in the endeavour to prevent abuses which may hereafter creep into the practice of vivisection of animals we do not afford facilities for the mental vivisection so graphically described in the evidence of Mr. G. H. Lewes, of honourable, kind-hearted and sensitive men, whose pursuits are not merely advantageous to science but productive, as the Report clearly shows, of great benefit, both to the human race and the lower animals. Legislation may still be wanted in the interests of physiologists themselves, not less than of the animals on which they experiment, but what we have said is, we think, sufficient to show that this must be undertaken in no hasty spirit.

MISS BUCKLEY'S HISTORY OF NATURAL SCIENCE

A Short History of Natural Science, &c. By Arabella B. Buckley. (London: John Murray, 1876.)

THE object of this book is, as stated in the Preface, "to place before young and unscientific people those main discoveries of science which ought to be known by every educated person, and at the same time to impart a living interest to the whole, by associating with each step in advance some history of the men who made it."

We are also told that—

"When treating of such varied subjects, many of them presenting great difficulties both as regards historical and scientific accuracy, I cannot expect to have succeeded equally in all, and must trust to the hope of a future edition to correct such grave errors as will doubtless be pointed out, in spite of the care with which I have endeavoured to verify the statements made.

"As the size of the book makes it impossible to give the numerous references which would occur on every page, I have named at the end of each chapter a few of the works consulted in its preparation, choosing always in preference those which will be useful to the reader if he cares to refer to them."

This last sentence fully accounts for the character of the work. We may say at once that we have not read it through. Our readers will probably soon see why. But we may get a good idea of its contents by dipping in here and there. Geologists do not require to break a mountain down into road metal in order to discover its structure. The book is by no means a paste and scissors production; it is evidently the outcome of very considerable mental exertion, not only in reading, but in thinking. But, unfortunately, the author seems to have accepted as equally trustworthy guides some of the best and most authoritative works extant, and along with them some, often of the trashiest, volumes of the popular scientific literature of the day. In several, especially of the earlier chapters, the first class of works is mainly referred to, in others the second; a few chapters are based upon a mixture, consequently the result is extremely curious and instructive, at least to the scientific critic.

As a whole, the work is decidedly superior to any of the popular ones on which part of it is based; though of course, as they have to a certain extent leavened it, it is in many places not only inaccurate, but positively astounding in its misrepresentations. It is quite easy, however, to trace in each case the more extraordinary blunders to their popular scientific source.

Recent British popular scientific works, at least since we have lost such masters as Faraday and Herschel, have, in general, one or other of two marked characteristics. The honest but ignorant man, too ignorant to be aware of his own ignorance, complacently and in good faith writes preposterous nonsense. Our author has wisely let *him* alone.

Other quasi-scientific men are acute enough to know, perhaps even to admit to themselves (but only under the strictest seal of secrecy) their own ignorance. Sometimes they may condescend so far from their pedestals as to seek assistance from those who are really competent to give it, but even then they do not save themselves. The true critic easily perceives by a single loose word or phrase which such writers cannot refrain from adding to the accurate periods of their mentor (if only to save their own consciences on the question of originality) the true state of the case.

When the quasi-scientific writer feels diffidence in asking assistance, he gets out of his difficulty by adopting what has been well called the "cuttle-fish dodge," and bewilders his readers by squirting in their faces a cloud of inky verbiage. Our author has trusted too implicitly to *him*.

Our readers must now have a notion of what seems to us, at least, the character of this book. The style is clear and good, and many of the incidental remarks and comments are happy. The early chapters, referring as they do mainly to subjects little treated by the modern popular science writers, are, as a rule, very much superior to the later ones, and in many places may be not merely passed as satisfactory, but even highly commended. For instance, the discovery of the law of compression of gases such as air, at constant temperatures, is, for once in a popular book, actually assigned to Boyle himself, though even here the pernicious influence of the popular quasi-scientific writers has asserted itself in the unwarrantable introduction of the perpetual Mariotte. There are some very sensible remarks on Werner and Hutton, and we are glad to see that William Smith's geological labours are heartily recognised. But what shall we say of passages like the following?—

> "We owe to him [Scheele] the discovery of chlorine; and of manganese, barytes, fluor spar, and many *other earths* whose names I cannot expect you to know."

Or this—

> "The determination of nitrogen completes the history of the discovery of those gases of which *fire*, air, and water are composed; but you will have noticed that we have not yet arrived at the new explanation of chemical changes which was to take the place of 'phlogiston.' The fact is that Black, Bergmann, Cavendish, Scheele, and Priestley, were all so cramped by the old theory, that though they discovered the facts they could not make the right use of them. *The* man who did this, and who laid the foundation of modern chemistry, was the celebrated French chemist, Lavoisier."

We have taken the liberty of italicising two words in each of these extracts.

Again, Fresnel "found that [Dr. Young] also had the same idea [interference], and this led to a number of experiments, by which they proved at last that the waves in a natural ray of light do not move *merely* up and down like waves in a pond, but also from side to side; and that when light is polarised this complex vibration is destroyed and the waves of each separate ray move only in one direction." The italics here are in the text!

From the later chapters we make but two extracts; these will, we fancy, be thought quite sufficient:—

> "If the water were free [in Joule's paddle experiment for the determination of the dynamical equivalent of heat] it would pass on into the air and we should lose sight of it; but the water is shut in and the force cannot escape, so now it employs itself in dashing to and fro all the little particles which make up the water, and producing the effect we call heat; and as it produces exactly 1° Fahr. of heat by the time the 1 lb. weight has fallen 772 feet, we say that *772 foot-pounds of force equals 1° Fahr. of heat.* You might easily prove to yourself in a somewhat unpleasant way that the force is there; for if you were to go on turning the paddle violently for many hours, and there were no means for the heat to escape, the motion of the particles would be so violent against the sides of the boiler that it would burst."

Here again the italics are not ours; but the whole passage shows clearly that the author has read this part of the subject in works in which scientific terms are used loosely or even inaccurately; while difficulties have been avoided, not explained. For when "we say that 772 foot-pounds of force equals 1° Fahr. of heat" we commit so many and such astounding blunders that it would be altogether impossible to enumerate them them all in such a notice as this.

Let us, however, do what we can in a few lines to point out a few of them. Suppose them put as questions by a teacher employing this work as a text-book.

1. What is a foot-pound of force?

To this question the late William Hopkins, perhaps the ablest instructor whom Cambridge ever produced, would have at once replied, "The old story; the height of King's College Chapel in acres!" When *will* our elementary writers at last recognise that a force may b represented by so many pounds' weight, but *cannot possibly* be represented by so many pounds' weight overcome through so many feet?[1]

2. What is 1° Fahr. of heat?

One might just as correctly ask, "What is 1 oz. troy of time?" We have heard of degrees of temperature, and of quantities of heat, but we are totally unable to conceive what could correctly be designated by 1° Fahr. of heat, though it is clear that it is here used for "the quantity of heat which can raise the temperature of a pound of water by 1° F." But let us proceed to our final extract:—

"If you have understood this explanation, you will have some idea of the theory that heat is altered motion; but to complete the history we require not only to turn work into heat, but also to turn heat into work. This had already been done many years before by a French engineer, M. Carnot, though he did not understand its real significance, but it has now been most beautifully proved by a long series of experiments made by M. Hirn, of Colmar, in Alsace. What M. Hirn practically did was to find out how much heat can be obtained from a ton of coals, and then to find out how much work was performed in an engine by that amount of heat. This was by no means a simple task, for much heat is lost in various ways in passing through the engine; and even when he thought he had allowed for all this, it was found that some of the steam had turned back into water on its way, and thus used up some of the heat. At last, however, when all was carefully measured and calculated, he found that *for every pound of water heated 1° F., enough work had been done to raise a weight of 1 lb. to a height of 772 feet.* This, you will notice, was exactly the converse of Joule's experiment, and proved that exactly as much motion is produced by means of confined heat as there is heat produced by means of checked motion."

This is certainly very novel information. We have hitherto been accustomed to think that "What M. Hirn practically did was [not] to find out how much heat can be obtained from a ton of coals," but to find how much of that heat disappeared by having been actually converted into useful work in an engine.

But enough has been said to show the necessity for the correction of those "grave errors" alluded to in the second extract made above from the Preface.

As to the chronological table with which the volume concludes, and which is carried on to 1874, we would only remark that it would not have been unduly extended if a little space could have been found for even a bare mention of a few such names as Andrews, Forbes, Graham, Stokes, Thomson, and Clerk-Maxwell in our own islands, and a few more like Helmholtz, Foucault, Plücker, and Weber abroad. Surely such names should have been caught at the very first cast of a net whose meshes were found small enough to seize Drebbel, Franklin, Celsius, Reaumur, Fahrenheit, and Humboldt!

HASSALL ON FOOD

Food: its Adulterations and the Methods for their Detection. By Arthur Hill Hassall, M.D. (London: Longmans, 1876.)

THIS book is practically a new edition of the author's former work on "Adulterations Detected in Food and Medicine;" the main difference being, at least so far as the plan of the work is concerned, that in the present volume it has been thought judicious to exclude the articles on drugs, which occupied indeed a very subordinate position in the older one. We cannot but commend the discretion thus shown: unsatisfactory as are many of the processes employed by the public analyst, viewed as methods of precision, none are more so than those applicable to the analysis of pharmaceutical preparations. It is probably to this fact that we must attribute the immunity from the raids of the inspectors which the apothecaries have on the whole enjoyed; otherwise we must assume that a higher standard of commercial integrity prevails with our druggist than with our grocer or milkman, a supposition to which, possibly, the milkman and the grocer would demur.

Despite the omission referred to, the present volume is nearly double the size of the former one; the increase in bulk may indeed be taken as commensurate with the increase in importance which the subject has of late years assumed. The additional matter comprises articles on the Function, Quantity and Preservation of Food, and on the influence of the utensils employed in its Preparation and Storage: on Unwholesome and Diseased Meat; and on Water and its Impurities A considerable quantity of fresh analytical work has been incorporated, and the author has been at the pains to test the greater number of the various methods commonly employed in the detection of adulteration, and in the determination of its amount. In one or two instances, as for example, in the determination of milk-residues, he is scarcely just to his contemporaries. The chemistry on the whole is fairly good; thanks to the cooperation of Mr. Otto Hehner, whose assistance Dr. Hassall freely acknowledges. But we are inclined, in common, we are sure, with those who have attempted to make absolute alcohol, to doubt that this liquid is best prepared by digesting spirit of 90 per cent. with well-dried chloride of calcium (p. 795). It is well known that calcium chloride unites with alcohol to form a compound in which the alcohol plays the part of water of crystallisation, and which is only decomposed with difficulty. Much is said respecting the composition of "fusel oil" and "potato spirit," although neither the author nor his *collaborateur* thinks it necessary to indicate anything with respect to the existence of isomerism in the alcohols mentioned. On p. 676 we see Payen and Chevallier's analysis of hops given: on the following page precisely the same analysis is repeated as indicating the composition of crude lupulin, which we are further informed, amounts in good samples to about one-sixth of the weight of the hops.

The public generally, and Messrs. Allsopp and Co. in particular, will be alarmed to learn that the water used in the brewery of that eminent firm contains 7·65 grains of sulphate of zinc to the gallon! In the face of such a statement it is hardly sufficiently reassuring to be told that "the water is remarkable for its complete freedom from organic matter" (p. 681). The memory of the strychnine "scare" in connection with the national beverage has scarcely died out: we trust that Dr. Hassall will be the first to allay our anxiety as to the existence of white vitriol in our "pale," and "Burton."

A considerable section of the book is devoted to the

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[1] We can hardly blame our author for this blunder, great as it is, when we find substantially the same in a recent work by so learned and careful a writer as Guthrie. In § 129 of his "Magnetism and Electricity" we are surprised to read that a body falling to the ground "will, just before reaching the ground, have acquired a MOMENTUM, which is equal to the WORK done in lifting it." The small capitals are ours—and they but faintly express the agony with which we peruse such a passage.